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At any rate, any possible conquest by the Chinese or Japanese would be preferable to the "Weltmacht" of the German.

While the introductory notes by Dr. Woods are very suggestive and original, the present reviewer cannot bestow the same praise on the part of the work accredited to Mr. Baltzley. The statistical summaries and diagrams prepared from various treatises and text books on history are comparatively crude in their makeup. Nowhere does the reader find even the slightest attempt at a mathematical and quantitative analysis of the data. The chronological tables as prepared by Mr. Baltzley do not represent original research beyond that of a simple tabulation. Probably the author has something better in store for us, but if this be the case, he ought to have indicated it in the text. Mr. Baltzley's comments on Poland are of interest; but the downfall of the Polish power must be sought for in a quite different direction from that of pure military extinction. The writer believes it to be a fact that all of the former great nations of Europe, such as Spain, Portugal, Poland and Turkey began to decline as soon as the Jewish element exceeded a certain percentage of the total population; and history seems to be repeating itself in the case of the rapidly disintegrating Austrian Empire, in which the Jews, by their financial strength and by their proportional strength in population, have exerted a tremendous influence. This is a very interesting phenomenon which Dr. Woods and his collaborator might study with profit.

ARNE FISHER.

REPLY TO DR. DAY'S CRITICISM OF MY ARTICLE ON CORRELATION COEFFICIENTS.

It would be unwise to occupy too much of the valuable space of "The Publications" with a lengthy discussion, but Dr. Day's well-stated criticisms are so broad in scope that they seem to call for a brief reply.

He first dissents from my observation as to the frequency of the misuse of the coefficient by reputable statisticians. I hope that he is right and that the cases of that type coming under my observation are fewer than my memory indicates. The instances in question are not at hand at present, but vivid recollections come to mind in which economists who habitually present statistical studies to the public have used coefficients to prove correlation between the cyclical fluctuations of two variables without getting rid of the trend or the lag. However, the question as to the standing of writers misusing the coefficient does not seem to me to be one of importance. My criticisms were intended entirely for the benefit of those who did not use the coefficient correctly. Experts in correlation would, of course, not need any direction concerning its application or interpretation.

Statistical literature has, however, for example, been too commonly favored with contributions in which a coefficient has been used to bolster up an assumption of correlation between short-time fluctuations when the only reason for the high coefficient was the fact that both variables had similar trends which had not been eliminated. In such instances, of course, the data would give a high positive coefficient whether the correlation between the shorter oscillations was direct, inverse, or entirely non-existent.

The tendency to assume that the high coefficient shown by two variables with similar steady trends is a proof of correlation between the two is probably the most flagrant misuse of the coefficient. As a matter of fact, Pearson's coefficient is entirely without significance for comparing historical variables unless there are a *considerable number* of corresponding up-and-down swings in the variables. If such corresponding swings occur, the plotted curves usually reveal the fact quite clearly. My own rule is that when the correlation is not entirely evident from the curves, it is unsafe to assume that correlation exists. When an examination of the curves leaves one in doubt, the coefficient, if computed correctly, will normally be very low. If the curves seem distinctly to indicate correlation, it certainly does no harm to clinch the evidence by appealing to a high coefficient to prove how successful one has been in eliminating the extraneous factors. It should, however, always be kept in mind that two trends are likely to be similar because of wholly unrelated causes; that in such instances, Pearson's coefficient will show very high correlation with a low probable error; and that, despite this apparently conclusive mathematical evidence, one is wholly unjustified in assuming any trace of interdependence or inter-relationship between the two variables. Two complex curves may be so similar that, with no other evidence whatever, one may be fully justified in concluding that they prove causal relationship. On the other hand, a large coefficient with a small probable error only establishes a similarity between the dominant tendencies of the two variables and, without exception, requires an examination of the plotted graphs or additional evidence from other sources before any assumption of a causal relationship is justifiable.

Dr. Day next takes exception to my definition of correlation. I still fail to see that I have in any way essentially changed the idea as it has been generally understood by statisticians. I have only sought to make definite and clear cut a concept which is frequently described in a more or less nebulous and hazy manner. And it still seems to me that "a tendency toward persistent association," as he puts it, between varying phenomena, must be either chance or causal. The former may interest the mathematician—the latter only is of significance to the economist. In my opinion, the latter relationship is the only one in the mind of the economist who refers to correlation. If a given cause always produces a definite effect which proposition Dr. Day does not dispute, then correlation is always perfect no matter how hard it may be to isolate and detect it. The different blind men who described the elephant so diversely did not by their description alter by one iota the characteristics of the elephant.

I cannot agree that the fact that correlation is always perfect, if existent, makes the problem of demonstrating the relationship between two variables necessarily a simple one. Extraneous factors sometimes are hard to eliminate, but only when these are disposed of successfully can the coefficient or any other method really prove correlation to exist. With other factors still present, a high coefficient indicates merely spurious correlation, due probably to the accident of similar trends.

The use of coefficients, therefore, is not in any sense a legitimate substitute for the elimination of the effects of factors other than those which it is desired to compare. If the troublesome interfering causes cannot be scientifically disposed of, to the required degree, then the use of coefficients gives no aid whatever, and we are compelled to concede defeat and pass the problem on to our successors.

I agree fully to "the practical impossibility of isolating economic forces *completely*." Neither are chemical or physical forces ever *completely* isolated. Nevertheless, it must be admitted that physical and chemical laws have been derived and established which are accurate enough to serve the ends in view. Identically similar conditions rule in the economic field except that the difficulties of isolation may, in some instances, be even greater than those besetting the physicist or chemist. I would be the last to assert the present possibility of solving correctly nearly all questions of correlation of economic facts, but fortunately my own experience with scores of typical economic problems demonstrates conclusively that it very often is perfectly feasible to eliminate all undesired influences to the extent necessary to show conclusively whether correlation does or does not exist. The process of segregation having been carried thus far, the facts are so evident that it is wholly immaterial whether they are presented in the form of coefficients or curves. To my mind, the most important single criterion of success for the statistical economist is his ability to isolate to the necessary degree the forces in which he is interested. The method of measuring these relationships is decidedly of secondary importance.

In conclusion, I wish to thank Dr. Day for his courteous comments. I also wish to give assurance that while my criticism concerning the use of the coefficient was not entirely confined to amateurs, I have had no thought of indicting all statisticians indiscriminately. I also concede freely the fact that the correlation coefficient, when intelligently used, is a perfectly satisfactory tool for measuring one's success in segregating a cause and its effect from a mass of confusing data.

WILLFORD I. KING.